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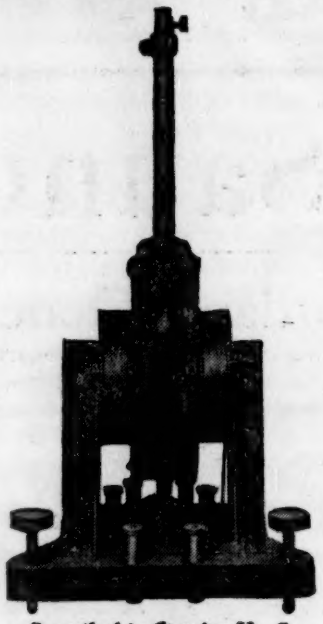

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THE CONTRIBUTIONS OF ZOOLOGY TO HUMAN WELFARE[1]

To indicate the contributions of zoology to human welfare as related to or dependent upon the aquatic resources is the task which has been assigned to me, but because of time limitations the subject must necessarily be covered in a superficial way. The consideration of this phase of the general topic, whose application may for present purposes be restricted to the United States, is based on the assumption that the condition of aquatic resources affects our national prosperity, and that full and accurate knowledge of those resources is a prerequisite to their proper utilization.

The theme is fertile and inviting, and there should exist no difficulty in establishing a case for zoological research as a noteworthy contributor to our welfare. My task is lightened by the readiness with which nearly every one will recall important rôles that zoology has played in the modern history of the fishing industry.

It is doing no violence to truth or justice to claim that the beginnings of sane and beneficent fishery administration in the United States date from the time when a man, already eminent in science, with many years' experience in zoological work, was chosen by the President of the United States as the proper person "to prosecute investigations on the subject of the diminution of the valuable fishes with a view of ascertaining whether any and what diminution in

[1] Read before Section F (Zoology) of the American Association for the Advancement of Science at a symposium upon "The Contributions of Zoology to Human Welfare," Pittsburgh, Pa., December 31, 1917.

the number of the food-fishes of the coasts and the lakes of the United States has taken place; and, if so, to what causes the same is due; and also whether any and what protective, prohibitory or precautionary measures should be adopted in the premises."

Since that year (1871) when Baird began those masterly and far-sighted inquiries, zoology has been the constant forerunner or associate of fishery progress; and the conservation of the resources of our lakes, rivers and coastal waters became an established policy and an accomplished fact many years before the term conservation came into general use as applied to any other resources.

At the outset of that pioneer movement, it was recognized that the only rational basis for the administration of the fisheries was a complete knowledge of aquatic creatures to be acquired by intimate investigation. The early researches, which have served as models for subsequent work, were expected to yield practical results but were conducted without any sacrifice of the cherished principles of science. In fact, Baird had the courage, which some persons might have regarded as temerity, to insist in his communications to Congress and the general public that in the elucidation of the economic problems which Congress had imposed it would be of doubtful value to study only the major forms which supported fisheries and that "useful conclusions must needs rest upon a broad foundation of investigations purely scientific in character."

Baird's reputation and official position and the attitude with which he approached his tasks enabled him to draw to his aid a large body of men trained in the methods of zoological research; and the great success of his early investigations, as of the later work that has been their continuation or outgrowth, was largely, in many instances entirely, dependent on the services of zoologists, most of whom were university men. I need only mention the names of Birge, Brooks, Bumpus, Dean, Evermann, Forbes, Gilbert, Goode, Grave, F. H. Herrick, Jordan, Kellogg, Lefevre, Linton, Mead, Parker, Rathbun, Reighard, Ryder and Ward to recall to you zoological work that has made an impress on the public welfare and entitles them, and others whose names will occur to you, to the thanks of a republic which has become more and more grateful as the knowledge of their work has spread.

It was the work of Baird and his associates in zoology that chiefly induced Huxley to assert his belief that no nation at that time had comprehended the question of dealing with the fisheries in so thorough, excellent and scientific spirit as the United States.

Brief reference may now be made to a few special cases out of the many that might be cited in which zoology has rendered noteworthy service. Passing over the high degree of perfection that has been attained in various branches of governmental and private fish culture, largely as a result of embryological and physiological studies, attention may be invited to the American oyster, which because of its prominence as our principal water product has deservedly received consideration at the hands of some of the leading zoologists. I need only recall the work of Brooks and Ryder who, with others, brought their highly developed scientific minds to bear on the practical problems of the oyster industry and, through their studies of the biology of the oyster and from experimental work in oyster rearing, rendered conspicuous and enduring aid.

With the oyster, as with other water creatures, the teachings of zoology have been at complete variance with the confirmed practises and deep-seated prejudices of certain states. The welfare of their

oyster industry was for a long period neglected; and the delivery of these states from the thraldom of obsolete, inefficient and wasteful methods has depended on their eventual willingness to accept zoological facts as the basis for administration.

Another of our great aquatic resources that has suffered from the failure or refusal of the states to be guided by the teachings of zoology is the lobster. If the states had given heed to the elementary needs of the lobster as proclaimed by Herrick, instead of shaping their course so as to conform with the interests of those who for years have been profiteering at public expense, they could have made the lobster a staple, moderate-priced food for all time, whereas it has become such a rare and expensive article that the food administration might very properly place an embargo on its use as a wholly unjustifiable extravagance. One alleviating circumstance is that, through the adoption of a system of artificial rearing devised by the zoologist Mead, the lobster supply in the waters of Rhode Island has been maintained better than in any other state.

One of the most noteworthy cases of the application of zoology to the public good is the prompt use that the government made of Lefevre and Curtis's investigations of the habits of the glochidia of the pearly mussels. The practical problem here presented was the maintenance of an industry that supports many thousands of people and directly affects every man, woman and child in the United States. It should be remembered to the everlasting credit of our national law-making body that it was willing on purely zoological claims, which were the only ones that could be put forth at the time, to establish an expensive station and adequate personnel for applying the results of zoological researches and experiments. The influence of that laboratory on the policy of the interested states has been great; the immediate results of practical value have been conspicuous; and the way has been made plain by which this great national resource, which far surpasses in value and volume that of all other countries combined, may be preserved through proper utilization.

Reference should be made to the extraordinary advance in knowledge of the age and growth of fishes that has come from recent studies of their scales and bones, and to the opportunity that is thereby afforded for the first time to substitute facts for guesswork in formulating protective fishery laws bearing on the size and age of food and game fishes.

A year or two ago there arose a situation in one of the largest seaboard cities where ill-advised administrative action threatened to exclude from the market one of the most abundant and wholesome marine fishes, with consequent disturbance of long-established trade and serious loss to the fishermen even of remote regions. A real disaster impended because a market inspector saw certain parasites and misconstrued their significance. The evil was averted by the ability of the government to recommend to the city authorities a zoologist with a most convincing mass of zoological evidence, with the result that the embargo was promptly lifted and in all probability will never again be placed in that community for such a reason.

The great contributions that zoology has made to all branches of fishery work have not exhausted the field. Dependence must continue to be placed on zoological research for the elucidation of the greater problems that are looming. A hopeful sign of the times is that the public now appears to be more willing than ever before to defer to and depend on the recommendations of zoology in the handling of fishery questions.

BUREAU OF FISHERIES, H. M. SMITH
WASHINGTON, D. C.

THE VALUE OF ZOOLOGY TO HUMANITY

The task assigned me is to present to you as adequately as may be possible in the brief time allotted this topic, the significance of studies on animal parasites for the progress of the human race.

Since the earliest times of unwritten history human records bear evidence to the bitter warfare waged by men of all races against the large carnivores that threatened their existence and played havoc with the useful animals they had brought under control. As time went on and his purview gained in breadth man turned his attention to noxious types such as the snakes, and to smaller forms like the rats that are real though less conspicuous foes. And, finally, in very recent years, he has learned to understand that minute organisms like the blood-sucking insects are not only sources of discomfort but also play an essential part in the transmission of many of the microscopic organisms that produce disease. He knows that these organisms both plague and destroy the domesticated animals, that they greatly reduce his own efficiency and, threatening his very existence, drive him away from rich and fertile territory or in a single epidemic wipe out families, villages, or even whole nations. In short, animals nowhere affect man more extensively and more seriously than in those relations in which they appear as the agents in producing or transmitting disease, and it is of fundamental importance for the progress, prosperity and even existence of the human race that those relations should be investigated, determined with precision, and brought under control.

This in a broad general way is the problem of parasitology, and it is important at the very outset to indicate that it is not set off from other studies on animal life by any hard and fast limits. The structure of parasitic animals can be interpreted rightly only by the results of knowledge gained concerning the anatomy of free-living forms; the development of parasitic species depends for its explanation on the results of studies on the development of animals generally; the habits of parasite and host are so intimately interlocked that the key to the life history of a parasite is found in the associations of the host. Now the recognition of this intimate and necessary connection between zoology in the narrower sense and parasitology is of relatively recent date and has favored promptly and conspicuously the contributions of zoology to human welfare. Brief reference to the history of science may serve to make this point somewhat clearer than it appears in the general statement.

As a matter of fact parasitology is of the oldest phases of the study of animal life and its even yet faintly recognized relations to man. Diseases caused by animal parasites played evidently an important part in the economy of the nations of antiquity. The oldest medical works that have come down to us give intelligible records of the presence in man of such guests and in some cases recognizable descriptions of the parasites themselves. This study pursued under the direction of the priests and medical men was developed apart from other fields of zoology. As a result the knowledge thus gained reached more considerable proportions and was more widely disseminated at an early date than the general knowledge of animal structure or of animal function. To some extent the separateness of the two fields exists at the present date, to the disadvantage of scientific progress and human welfare.

The parasites known to the ancients were of course the larger, more conspicuous forms. And while some important facts were included, their knowledge was con-

fused with such a mass of fable and superstition that its value as a guide to a rational life was not large. Moreover these large forms are not the most dangerous parasites, *i. e.*, those of greatest significance for human existence; and hence in that period the contributions from the study of parasitology to human welfare were not conspicuous.

Something like a century ago students came to see, faintly and slowly at first, that animals affect man very directly because some of them are definite factors in the production or transmission of disease. One can hardly overestimate the importance of the contributions made by zoologists toward the solution of the problems presented in this field. At first the instances of animals as causes of disease appeared rare, and were recognized only among the higher parasites. In particular cases of this type a demonstration was achieved and the evidence became clear as scattered observations of individual workers were brought together by some master mind and formed into a consistent and sufficient explanation of the malady. A splendid illustration is furnished by the epoch-making investigations of Leuckart and others on trichina. Owen had found in a dissecting room in London a worm encysted in human muscle and had rightly interpreted it as a nematode; another form was discovered by Leidy in Philadelphia in pig muscle and recognized as identical with that reported by Owen; and, finally, adult intestinal parasites were obtained from cases in Germany which had been previously diagnosed as typhoid, and all these were shown to represent only different stages in the life history of a single parasite, the trichina. The natural result of these studies was the formulation of a rational prophylaxis which, when adopted, eliminates trichinosis absolutely from the list of human ailments.

These early and significant discoveries were accompanied by others of lesser importance on various other worms but remained after all relatively isolated for a long time. The reason is not hard to find. The rapid and brilliant rise of bacteriology to a position of prominence and its overwhelming demonstrations that bacteria were the causes of many diseases led investigators to seek insistently for them in all maladies of an apparently infectious type. Failure to find them was explained on the basis of defective technic and the possible existence of other disease-producing organisms was generally overlooked.

To be sure, malaria had been traced to a protozoon, but the instance remained isolated for many years until a combination of causes directed scientific attention to the importance of more exact studies on that and allied organisms, and these in turn opened up a new field, that of protozoal diseases. In the investigation of this field, zoological students have performed the chief and almost the only extensive work, and the results of their researches have been of tremendous significance to human progress.

In his "Malaria Studies of a Zoologist" Grassi laid the foundations for the complete elucidation of the life history of the *Plasmodium malariæ* and with that the basis for the adoption of communal and individual habits which have freed the world from the menace of a disease that has exacted a heavy toll from the great nations of all ages in human history. Coupled with this the demonstration of the rôle of the mosquito in transmitting yellow fever made possible for instance the building of the Panama Canal and the maintenance of a highly developed and cultured community on the Isthmus, whence less than half a century before the French workers had been driven out by pestilences so frightful

that no system of recruiting the workers could keep pace with the destruction of human life wrought among them by these diseases.

One should not pass without notice a group of protozoal and bacterial diseases transmitted by insects, such as sleeping sickness, bubonic plague and the various relapsing fevers, all of which owe their spread in nature exclusively to inoculation by bedbugs, ticks and biting flies. The significance of scientific discoveries which have led to formulation of measures for the control of such diseases may be estimated in correct propositions when one bears in mind that at a very recent date malaria has claimed in Italy alone 200,000 victims annually, sleeping sickness 500,000 in Africa and plague in India has often destroyed more than a million victims in the course of six months. Among those who have contributed prominently to the study of these organisms and to the solution of the problems of the diseases they produce must be named the zoologists Minchin and Fantham; of the work done on the insects that transmit such organisms appropriate mention is made by another speaker in this symposium.

Not less significant is the story of the hookworm and its ravages, which have extended over every part of the globe in tropical and subtropical regions and have justified the organization of a great philanthropic movement to combat them. The first great step in advance in the handling of this problem was taken when at the International Zoological Congress in Berne, Switzerland, the zoologist Looss demonstrated before a critical and unbelieving audience the intricate story of the migrations of this parasite and its mode of gaining entrance to the human host. Previous to that time, no effective measures for combating the disease had been worked out, save the procedure of abandoning infected territory which was in itself a clear confession of inability to cope with the difficulty. As a direct result of the definite knowledge concerning the life history of the parasite and its point of attack upon the human system, a code of procedure has been worked out so definitely that its introduction is in fact resulting in the elimination of the disease from human consideration. Foremost among those who have aided in achieving the brilliant results of the campaign against the hookworm stands the name of the zoologist Stiles.

When the city of Manila came under the control of the United States of America, one in four, if not one in three, of all the deaths occurring in its limits were due to the attack of the parasite of amebic dysentery. While it is premature to say that this problem has been solved, still, studies on the structure and life history of this and similar forms by the zoologist Schaudinn and others have contributed such information concerning the life history and the effects of this parasite as to indicate successful lines of defense against its attacks and to reduce greatly the toll of human life it exacts.

But these items do not indicate the extension of knowledge regarding such organisms. Even among the higher parasites studies of recent date have added to the list of forms that menace man several intestinal flukes, three blood flukes, a number of tapeworms and various roundworms, and there is no reason to think the list is complete. Each form demands an individual study of its structure, life history and habits before definite measures can be formulated for protection against its attacks. To recount all that has been done in the many cases would transcend the possible limits of this paper. A very large part of the significant information regarding these forms has been furnished by zoologists. When, however, protozoan parasites are taken into consid-

eration the field is vastly extended. Both in numbers and variety and probably also in virulence of attack they surpass the metazoan parasites. The volume of the literature dealing with such organisms has increased with great rapidity and new organisms as well as new problems are being discovered every day. When one adds to all this the important problems that concern parasites affecting domestic animals and thus the material welfare of the human race, the extent of the field of work on animal parasites can be to some extent appreciated. The mere listing of the names of those who have worked in this field fills pages in every annual bibliography of zoology.

Such examples as have been cited could be multiplied many fold. Some of the parasites are acknowledged to be serious foes of human progress and if in other cases the effects are less conspicuous because the organisms are confined within narrow geographical limits, still the significance of their control is no less important to the human race. Furthermore, with the development of commerce and the increasing movement of individuals from place to place, disease has lost its earlier local limitation and has become an international menace. To all students of social evolution disease looms high as a controlling factor in human affairs. History furnishes many records of epidemics that have depopulated great areas, routed victorious armies, and reduced virile and fruitful civilizations to an inefficient and sordid level. Throughout the expanse of the tropics, where nature rewards the efforts of the cultivator of the ground in a degree beyond all comparison with the results obtained in temperate climates, human social organization has generally remained on a low plane, or if at any point it took a brief spurt, the end of the advance came quickly and the nation sank to the common level again.

Man's failure thus far to achieve the conquest of the tropics may be traced distinctly to the ravages of tropical diseases unrestrained by natural limitations that are found in other climatic environments. Now these diseases are primarily those produced by animal parasites and in the control of such diseases lies consequently the possibility of utilizing the natural resources of the globe at the point of their greatest richness. What greater service could be rendered to humanity than the continued maintenance of highly organized social communities in the richest regions on the surface of the earth? The result already achieved in the Panama Canal zone and at many other individual points in the tropics can be duplicated generally. When this is done the possibilities of human existence will have been enormously enriched. Certainly the demonstration of such a possibility which has already been given in work quoted above should rank as the greatest achievement of zoological studies and inferior only to the realization of that possibility when the conquest of the tropics is completed because of the mastery of its diseases.

Before closing the discussion, I should like to call your attention to another phase which is much less conspicuous than those which have been treated before, but which in my opinion has distinct importance. This is the stimulus given by parasitology to the scientific discussions of biology and consequently to the extension of knowledge and to the general advantage accruing therefrom. Important side lights have been thrown upon morphology by study of the processes of modification and degeneration which are so conspicuously exemplified by parasitic forms. The life history and relationship of these types show many features

unique in themselves which serve to illuminate general biological principles. One can not for a moment doubt that in some way or other the processes of existence have worked striking modifications in the parasitic forms when one compares them with their free-living relatives and scans the changes which take place in the individual development. Not even the most extreme conservative would wish to maintain that the parasite was an original product, or would hesitate to grant that it had become adapted to its present mode of life. Yet more extraordinary and far-reaching structural modifications could hardly be asked for or found than those which are evident in the parasitic organisms. Nor would it be easy to conceive more intricate or more precisely balanced relations than those which exist between some parasites and their hosts. The development, modifications and habits of the parasite have been coordinated with the conditions of existence in the host in strikingly precise fashion. Investigation has as yet only begun to work out the adjustments which have arisen independently and in great variety in different species and groups of parasitic species. The field is one that offers unusual opportunities at the present time to the investigator.

We do not know how far an intimate study of these problems may carry us towards the explanation of the process of evolution in free-living organisms. There is reason to think that the change has been more rapid as well as more radical among parasitic species. And if so, the study of this problem at this point may be expected to throw welcome light on the factors that lead to structural changes in living organisms. Any such study will certainly serve an important purpose in broadening the human mind and encouraging it to seek the solution of the problems of existence more vigorously than it has even done as yet.

The outlook for the future constitutes no less than the achievements of the past, a real contribution to the cause of human progress.

Henry B. Ward
University of Illinois

THE INDIGEN AND CULTIGEN

If an author were to prepare a flora or manual of cultivated plants in any country, he would come hard against the fact that he deals with two gentes or types of species.

One gens has recorded origin, with the typical form well recognized and probably represented by a "type specimen" in the herbarium of the person who "founded" the species. It in an indigen of known habitat.

The other gens is a domesticated group of which the origin may be unknown or indefinite, which has such characters as to separate it from known indigens, and which is probably not represented by any type specimen or exact description, having therefore no clear taxonomic beginning. I trust I may be pardoned for calling such species or group a cultigen.

A good example of the cultivated indigen is *Thuja occidentalis*. Although there are many horticultural forms, their relationship is understood, we are familiar with the species in the wild, and we have the whole case before us. The variations under domestication are indeed great, but we readily range them with what we call the species itself.

A good example of the cultigen is *Zea Mays*. We know neither its country nor its origin. It is widely variable. If a botanist had before him good material of all these variations, I do not know what one of them he would take as "the type." It is a composite gens, with no clear taxonomic center from which variations diverge.

Here we have two classes of facts, with no adequate way of expressing one of them in taxonomy.

If *Zea Mays* were an isolated case we could treat it as an exception. I have before me a list of one or two hundred comparable cases,

and yet I have made no careful search. Botanical literature is full of cultigens, improperly or incompletely coordinated into taxonomic treatment.

The prime deficiency is the fact that many of the good cultigens are unrecognized botanically. In the presumed manual of cultivated plants, how would the author treat the tuberous begonia? Would he enter descriptions of the several indigenous species from which the cultigenous group has come, and stop there? But what, then, would the horticulturist do? He would say that *Begonia Veitchii, B. rosæflora, B. Davisii, B. Pearcei, B. Clarkei* are not in cultivation so far as he knows, and he asks what he shall call the tuberous begonia. He would charge that the tuberous begonia is left out, and his statement would be correct. If he is a dealer, he naturally and properly wants a name in his catalogue comparable with *Begonia Rex* and *B. semperflorens.* Voss solves this problem by calling the cultivated group *B. tuberhybrida.*

Now the botanist will say that *Begonia tuberhybrida* has no "type," no clear description properly published, and therefore no recognized taxonomic standing. It is essentially as good a case, however (except traditionally), as *Zea Mays,* which some persons now consider to be a bigeneric hybrid.

If we accept the Linnean and other historic cultigens, why not accept modern groups of similar or comparable origins? Are the following "good species" in the strict sense? *Triticum vulgare L., Hordeum vulgare* L., *Secale cereale* L., *Helianthus annuus* L., *Saccharum officinarum* L., *Pyrus Malus* L., *Ipomœa Batatas* Poir., *Abutilon pleniflorum* N. E. Br., *Lonicera americana* Koch, *Lilium japonicum* Thunb. and *L. testaceum* Lindl., and any number more. We have similar cases in the domestic animals, as *Felis domestica, Gallus domesticus, Canis familiaris.*

What are we to do with cultivated blackberries, ixias, gladiolus, fuchsias, and many of the magnolias, deutzias, spireas, pandanus, roses? What are we to do with the cultivated canna: what is this plant? Are we merely to pass it by, undescribed because it is a complex? To describe the various species of canna is of no consequence to its identification. At present there is no name under which we can describe the common garden canna. The point is, are we to name and describe cultivated plants or are we not?

What are we to do with such things as *Saintpaulia kewensis, Tritonia crocosmæflora, Iris flavescens, Ligustrum coriaceum, Eryngium Oliverianum, Fuchsia speciosa, Heuchera brizoides, Primula Polyantha?*

The cultigens are with us, and the numbers will increase. No longer can we let them go by default. The plant-breeder will bring his new groups; will taxonomy expand itself to receive them, or must they always be outcasts?

Even when the parent indigen is known, many of these cultigens have their own entity and by every taxonomic right should be separately recognized. They often present characters new, or different from those of the fundamental species, or at least in different combination. When recognized as admissible gentes, in the company of living things, they are no longer involved in debates as to the taxonomic merits of their ancestors. Even if we were satisfied to say that the cultivated blackberries are *Rubus allegheniensis,* what are we to say when *R. allegheniensis* is itself split into a dozen segregates?

Supose, now, we are to agree that *Zea Mays* is a hybrid of *Euchlæna mexicana* and *X*: are we then to describe *E. mexicana* and *X* in our manuals, and to say that Indian corn is a hybrid between them, dropping the name *Zea Mays* entirely? This is exactly the type of treatment we are giving great numbers of cultigens that have well-marked characteristics of their own. Many of our common cultivated plants can not be put in our manuals, because we have no names to call them by. What are we to call the florist's chrysanthemum? To describe its supposed parents, *C. indicum* and *C. morifolium,* is of no consequence; these are unknown to the cultivator, and moreover they are not the florist's chrysanthemum.

It is said that to admit such forms into the society of recognized species would greatly

disturb systematic botanical procedure. The replies to this position are two: (1) We have already admitted very many of them, even if under protest in some cases; (2) is botanical procedure to be competent to accept the facts of nature? Whether we will or no, these cultivated things will be known by botanical names. What are we to do with *Phlox decussata?* It may be a set of hybrids between *P. paniculata* and *P. maculata,* but we can not order the plant from the nurseries under either of these names. Referring the name *P. decussata* to one or the other of the species may satisfy the demands of synonymy, but it does not dispose of the plant. It is a good name for the group: why not use it?

Naturally we must have a formal and recognized system of taxonomy and nomenclature. We should keep it pure. But may it not be extensible? The interminable discussions over trivialities of priority in nomenclature tend to seal up the subject as a closed book, or as an ancient box of precious ointments. May we not open the book or carefully lift the lid?

I have no program. To-day I am only asking questions. I would not interfere in any way with the orderly procedure that we have found to be good. I would disturb nothing: but may we add?

May we not admit the cultigen, under well-considered practise of conservative and trained botanists, defended with proper safeguards? I am not thinking of mere variations, even if well marked, but of important groups or clans of known characteristics under domestication. If so, the gens should have standing, which means that the name should bear record of its author. Its name should have recognized botanical form, for cultigens are still plants and of more or less coordinate rank with other gentes known as species. While falling under recognized botanical procedure, might it not represent a category or class of its own? In the manuals perhaps its name would be set in a different type; or could a designating symbol be used? Under the International Rules, the cross-mark (x) preceding the name is recommended to distinguish hybrids; this can not be applied to any extent because records of hybrid origins are few; it does not touch the great class of cultigens: yet there must be some good way of distinguishing categories.

We must assuredly try to avoid confusion, but we do not accomplish this by avoiding the facts. Horticulturists as well as botanists are entitled to protection and precision. May we not make names for certain cultigens?

These may be troublesome questions, but they force themselves on us. Is it not best to meet them squarely, and provide a way?

If we can not modify our practise in these regards, there is no use of making a manual of cultivated plants.

L. H. Bailey

SCIENTIFIC EVENTS

THE RESEARCH COMMITTEES OF THE BRITISH INSTITUTION OF MECHANICAL ENGINEERS

Some particulars of the work of the research committees formed under the direction of the Institution of Mechanical Engineers are given in the report of the council for 1918 and are quoted in the London *Times.*

The alloys research committee has been occupied with investigations on various light ternary alloys. These investigations have been conducted at the National Physical Laboratory with the assistance of the Department of Scientific and Industrial Research from whom the committee received a grant of £400, in addition to £800 paid directly to the laboratory for the provision of special plant. The council of the institution made a grant of £250 for the year. The committee's eleventh report, which would have contained the results of these investigations, has been temporarily withheld in the public interest.

The committee on steam nozzles, which received a grant of £100 from the council, has been so fully occupied with war work that it has been unable to construct apparatus and carry out tests; but complete detailed working drawings of the apparatus for measuring the impulse of steam jets have been prepared, and it is hoped that the apparatus may shortly be put in hand.

Dr. Stanton, with his special machine at the

National Physical Laboratory, has continued the series of tests for the committee on hardness tests, with special reference to the effects of variations of load and speed on rate of wear. A series of wear rings of varying widths has been made from material supplied by Sir Robert Hadfield, who has also undertaken their hardening. Another holder for these rings has been constructed of a form which will considerably facilitate regrinding. A series of specimens has been prepared and the tests are now in hand. The work was delayed for some months owing to some of the apparatus being required for war work. In addition to the grant of £100 made by the institution, a sum of £100 has been received from the Department of Scientific and Industrial Research, Sir Robert Hadfield has placed in the hands of the institution a sum of £200 to be awarded as a prize or prizes for the description of new and accurate methods of determining the hardness of metals, especially those of a high degree of hardness, but the council regret that as yet few such descriptions have been received.

The work of the committee on wire ropes, to which a grant of £450 was made by the council, has been much delayed by war work and the prolonged illness of the chairman. Nevertheless a design for a testing machine has been approved in principle for giving a somewhat wider range of tests than was originally contemplated, in the direction of providing for more bends both simple and reverse, and also for bends in planes at right angles. The choice of a site for its erection has been deferred.

In connection with the offer of a gift of £500 from Mr. Richard Williamson in aid of engineering research, a number of suggestions for subjects were received. The one which the council selected was on the best form and material for pistons and piston-rings, especially for internal combustion engines, and they are awaiting the approval of the Department of Scientific Research through which Mr. Williamson's offer was transmitted.

A PREHISTORIC PUEBLO INDIAN RUIN

THE American Museum of Natural History, in the summer of 1916, entered upon the largest single piece of scientific excavation ever undertaken in the United States. This was the systematic excavation and reparation of one of the finest and best preserved examples of prehistoric Pueblo architecture in the Southwest. The ruin is located in the Animas Valley in northwestern New Mexico, a few miles below the Colorado boundary and directly across the river from the town of Aztec, and is popularly, though inaccurately, called the "Aztec Ruin." It is the property of Mr. H. D. Abrams, of Aztec, who has given the Museum a concession to clear out and investigate the entire ruin. The funds for carrying on the work have been contributed by Messrs. Archer M. Huntington and J. P. Morgan.

The "Aztec Ruin" was once a typical pueblo, or great fortified house and village, comparable in the number of people sheltered to the modern American apartment house, but differing from it in that the principle of the pueblo was close communal cooperation. The buildings were so joined as to enclose three sides of a rectangular court whose fourth side was protected by a low, outcovering wall. Only one entrance led through the outer wall into the pueblo, which was, therefore easily defended. The three buildings, rising sheer from the ground on the outside, with very small windows, rose within the court by receding steps, each a story high. Interior stairways were not in use, access being gained to upper levels by movable ladders. As a military contrivance, this plan could hardly have been improved upon, since an enemy would be forced to make not one, but a series of attacks, to get possession of the building.

Although the work of investigation has as yet been only partially completed, the features of the ruin itself, and the surprising finds which have been made within the crumbling walls, have proved of sufficient importance to surpass the most sanguine expectations of the investigators. Necklaces of shell and turquoise, agate knives, pottery vessels of varied form and ornamentation, cotton cloth and woven sandals are among the gems of prehistoric Pueblo art which have recently been

unpacked in the laboratories of the American Museum. The work has been supervised by Assistant Curator N. C. Nelson, under the immediate direction of Mr. Earl H. Morris, also of the American Museum.

The seventy thousand specimens already recovered from the Aztec Ruin constituted one of the most complete collections representative of a prehistoric North American culture which have thus far been obtained. Trained preparators are working with the material, and in the near future a representative selection will be placed on exhibit in the Museum's Southwest Hall.

One of the most important phases of the explorations at Aztec is the repair and preservation of the ruin. As fast as the walls are uncovered, masons replace the stones which have disintegrated, and strengthen the portions of the structure which threatened to collapse. The intention is to make of the ruin a permanent monument to the aborigines of the Southwest rivalling in importance the Mesa Verde National Park.

THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY AND THE McKAY BEQUEST

President Richard C. Maclaurin in his annual report to the corporation of the Massachusetts Institute of Technology in referring to the recent decree of the Supreme Court with reference to the agreement between the Institute and Harvard University says that this agreement marked an epoch in the history of educational progress in this country. The end sought was to build up an educational machine more useful to the community and to the nation than anything that could be maintained by either the institute or the university, acting independently. Dr. Maclaurin writes:

> The plan adopted by the two corporations nearly three years ago has in the meanwhile been put to the actual test of experience and has met that test well. Most, if not all, of the difficulties that were anticipated by some have either not presented themselves at all or have been easily overcome. The educational power both of the institute and the university has been greatly strengthened and the cause of science that is applicable to the service of man greatly promoted by this combination of forces. . . . Unfortunately, however, the funds that the university has at its disposal for the promotion of the great science of engineering are almost wholly dependent on the income from the Gordon McKay Endowment, and the Supreme Court has decreed that this income can not be applied in the manner indicated by the agreement. . . .
>
> It remains to be seen whether another plan can be drawn up that is equally or nearly equally, workable and effective as an educational instrument and that accords with the view of the court regarding Mr. McKay's intentions. We should be false to our educational trust if we did not give this matter due consideration and earnestly seek a satisfactory way out. If intimate cooperation between these two institutions was demanded by the exigencies of the situation before the war, it is still more urgently demanded now. With the serious problems that this nation must face during the war and the equally serious problems that must be dealt with in the period of reconstruction thereafter, needless duplication of effort and needless dissipation of energy would be in a high degree reprehensible. . . .
>
> As far as the institute is concerned in the near future the abandonment of this agreement would be much less serious in its financial aspects than seems generally to be supposed. This arises from the fact that the actual amount of income available from the Gordon McKay Endowment has been greatly exaggerated in certain quarters. According to the testimony before the court, all that the university has available at present is the income from less than two and one quarter millions. Under the agreement Harvard does not turn any of this income over to the institute, but appropriates a portion of it for the maintenance of courses leading to Harvard degrees, these courses being conducted at the institute. The amount thus appropriated since the agreement went into operation has been $100,000 annually, the major part of this having been employed in paying the salaries of the university's professors and instructors. The whole amount is less than one tenth of the annual expenditure of the institute. It must not be supposed, therefore, that the institute will be crippled financially if the agreement with the university is abandoned.

THE GENERAL MEETING OF THE AMERICAN PHILOSOPHICAL SOCIETY

The American Philosophical Society will hold its annual general meeting at Philadelphia on April 18, 19 and 20. Dr. William B.

Scott, professor of zoology at Princeton University, will preside, succeeding Dr. W. W. Keen, who after ten years of distinguished service would not permit himself to be re-elected.

The general lecture will be given in the hall of the Historical Society of Pennsylvania on the evening of April 19, by Lieutenant Colonel R. A. Millikan, of the Department of Science and Research of the Council of National Defence, whose subject will be "Science in relation to the war."

The annual dinner will be held at the University Club, on the evening of April 20.

On the afternoon of April 20, there will be a symposium on "Food-problems in relation to the war" the program of which is as follows:

Introductory Remarks, by Herbert C. Hoover, B.A., U. S. Food Administrator, and by Alonzo E. Taylor, M.D., professor of physiological chemistry, University of Pennsylvania.

"Physiological effects of prolonged reduced diet on twenty-five men," by Francis G. Benedict, Ph.D., ScD., director of the Nutrition Laboratory of the Carnegie Institution of Washington.

"Food conservation from the standpoint of the chemistry of nutrition," by Henry C. Sherman, Ph.D., professor of food chemistry, Columbia University, New York City.

"Some economic aspects of the American food supply," by J. Russell Smith, Ph.D., professor of industry, Wharton School of Finance and Commerce, University of Pennsylvania.

"Food control and food conservation in the United States Army," by John R. Murlin, major, Sanitary Corps, N. A.

SCIENTIFIC NOTES AND NEWS

AT a meeting held on March 19, the Academy of Natural Sciences of Philadelphia elected as correspondents, John H. Comstock, Herbert S. Jennings, Frank R. Lillie, Alfred G. Mayer, John C. Merriam, George H. Parker and Charles R. Van Hise.

AT a meeting of the Rumford Committee of the American Academy of Arts and Sciences held on March 13 last the following grants for research were voted: To Professor F. K. Richtmyer, of Cornell University, in aid of his researches on the optical properties of thin films (additional to a former appropriation), $500. To Professor Arthur L. Foley, of the University of Indiana, for his research on the photography of the electric spark at different periods of its history, $150. To Professor Orin Tugman, of the University of Utah, for his research on the conductivity of thin metal films when exposed to ultra-violet light, $100.

THE following fifteen candidates have been selected by the council of the Royal Society to be recommended for election into the society: Charles Bolton, Henry C. H. Carpenter, Thomas A. Chapman, Gerald P. L. Conyngham, C. Clifford Dobell, Ernest Gold, Henry B. Guppy, Albert G. Hadcock, Archibald V. Hill, James C. Irvine, Thomas Lewis, Srinivasa Ramanujan, Arthur W. Rogers, Samuel Smiles and Frank E. Smith.

THE Paris Academy of Sciences has elected two national correspondents for the sections of anatomy and zoology. M. Vayssière, professor of the faculty of sciences at Marseilles, has been elected to take the place of the late M. Renaut, and M. Cuénot, professor at the University of Nancy, has been elected to take the place of the late M. Maupas.

THE introduction of compulsory rationing in Great Britain and the discontinuance of the voluntary propaganda department has led the food controller to reorganize the food economy division of the ministry hitherto conducted by Sir Arthur Yapp. It will now consist of four branches: public services food consumption, national kitchens, public catering, and an educational branch under the direction of Professor E. H. Starling, F.R.S. The coordination and control of the departments will be exercised by a Food Survey Board, of which the directors of the several departments will be members, with Lieutenant Colonel A. G. Weigall, M.P., as chairman.

PROFESSOR W. W. WATTS, professor of geology at the Imperial College of Science and Technology, has been elected a member of the Athenæum Club, London, for "eminence in science."

Mr. R. Bullen Newton, of the geological department, British Museum (Natural History), has completed fifty years active government service. During the earlier part of his official career, which commenced on January 6, 1868, Mr. Newton was one of the assistant naturalists of the Geological Survey under the late Professor Huxley. He was transferred to the British Museum in August, 1880, at the time of the removal of the Natural History Collections to Cromwell Road.

Professor C. H. Lees has been elected president of the Physical Society of London. The Vice-presidents are Professor J. W. Nicholson, Professor O. W. Richards, Dr. S. W. J. Smith and Dr. E. W. Sumpner.

Professor Tuffier, of Paris, has been promoted to the rank of commander of the Legion of Honor in recognition of his eminent services as consulting surgeon to the French armies.

Major J. G. Fitzgerald, associate professor of hygiene and director of the Connaught and Antitoxin Laboratories in the University of Toronto, has left for active service overseas in the Royal Army Medical Corps, having been transferred from the Canadian Army Medical Corps.

Dr. William P. Wood, assistant professor of chemical engineering at the University of Michigan, has resigned, to join the Signal Corps of the Army.

Captain Lawrence Martin, National Army, ordinarily associate professor of physiography and geography at the University of Wisconsin, is on duty in the Military Intelligence Section, War College Division, Office of the Chief of Staff, War Department, Washington, D. C. He has charge of the map room at the War College and of the maps in the offices of the War Council and of the Chief of Staff, and does geographical work for the General Staff in the combat branch of the Intelligence Service.

According to the *Journal* of the American Medical Association Dr. Edgar M. Green, of Easton, Pa., a member of the advisory board of the state health department, is mentioned as the probable successor of the late Dr. Samuel G. Dixon, health commissioner of Pennsylvania.

F. B. Howe, M.S. (Iowa State College '16), has accepted a position as land classifier with the U. S. Geological Survey.

On the evening of February 20, Dr. Edmund Otis Hovey, curator of geology in the American Museum of Natural History, gave a lecture at Mount Holyoke College on the subject, "Two years' experience in the Arctic with the Crocker Land Expedition."

Professor H. C. Sherman, of Columbia University, spoke to the Virginia Section of the American Chemical Society at Richmond, on March 15, and at the Randolph-Macon Woman's College, Lynchburg, on March 16, on "The food situation from the viewpoint of nutrition."

The fourth Guthrie Lecture of the Physical Society of London was delivered on March 22, at the Imperial College of Science, South Kensington, by Professor J. C. McLennan, of the University of Toronto. The subject was "The origin of spectra."

The *Journal* of the American Medical Association states that the attention of the Surgeon-General of the United States Public Health Service has been called to the fact that men from the military service who are carriers of various infectious diseases, particularly meningitis, have been discharged into the civil communities of the country. Dr. Oscar Dowling, president of the Louisiana State Board of Health, has made a particularly strong protest against this action by the government authorities, calling attention to specific instances in which meningococcus carriers have been discharged from the service.

UNIVERSITY AND EDUCATIONAL NEWS

A fellowship in physiological chemistry has been established at the University of Chicago by the Fleischmann Company, of Peekskill-on-Hudson, New York, for the purpose of in-

vestigating some of the scientific questions which have arisen in the course of the manufacture of compressed yeast under present war conditions. The university has appointed the first fellow on this foundation, who is now engaged in research upon the problems.

DR. WILLIAM M. JARDINE has been appointed president of the Kansas State Agricultural College and entered upon his duties on March 1. Dr. Jardine had been connected with the college for about eight years, first as professor of agronomy and for five years as dean of the division of agriculture and director of the Agricultural Experiment Station.

DR. F. E. DENNY, of the University of Chicago, has been appointed research assistant in horticulture in the Oregon Agricultural College, to fill the vacancy left by the resignation of Mr. Magness, the appointment to take effect on April 1.

DR. HELEN M. GILKEY, of the University of California, has been appointed assistant professor of botany and curator of the herbarium in the Oregon Agricultural College, to succeed the late H. S. Hammon.

DR. ETHEL M. TERRY, of the department of chemistry of the University of Chicago, has been appointed to an assistant professorship.

DR. FRED W. UPSON, for the past four years professor of agricultural chemistry in the Nebraska College of Agriculture, will, on June 1, become head of the department of chemistry in the University of Nebraska. A chemical laboratory which is modern in every respect, will be ready for occupancy at that time.

DISCUSSION AND CORRESPONDENCE

AN APPARENTLY NEW PRINCIPLE IN THE FLOW OF HEAT

SUPPOSE a number of horizontal metallic strips are maintained at constant temperatures, the first one at a low temperature, the next at successively higher temperatures, the last one being at the maximum temperature of a Bunsen flame, say a white heat. Let them all be of the same metal and have like surfaces.

Now suppose the same Bunsen flame be applied under like conditions to each strip. According to text-books and the laws of the transference of heat as usually taught, one would be led to believe that the coldest one should absorb the heat from the flame most rapidly, the next one less rapidly, and so on. Tests made by the writer, however, show this to be an error and that up to a certain high temperature exactly the reverse is the case; the coldest one will absorb the least amount of heat from the flame, the next hotter one will absorb more and so on up to a temperature at which the rate of absorption will be a maximum, after which it diminishes again, becoming zero for the one whose temperature is equal to that of the flame.

As stated in the premises, the heat which enters the metal from the flame is supposed to be conducted away as fast as it enters, and it is this heat which is measured. This could be carried out by using flat-bottomed iron cups or crucibles containing various materials having successively higher but fixed boiling points, say like liquid air, water, sulphur, zinc, etc.

When a very hot gas, like that in a flame, impinges on a relatively very cold surface from which the heat is led off as fast as it enters, like in the boiling of water by flame heat, a peculiar phenomenon takes place in that the equivalent of a very thin film of extremely high thermal resistance is formed on the surface exposed to the flame. Considered as a thermal resistance, the writer finds that for a constant temperature flame its resistance decreases rapidly as the temperature of the absorbing surface increases, contrary to what would have been supposed. The transmission of heat therefore increases as the absorbing surface becomes hotter and reaches a maximum which appears to be roughly when the drop of temperature from the flame to the surface is equal to that from this surface to the constant temperature boiling liquid; the transmission then must fall again, becoming zero when the temperature of the boiling liquid is equal to that of the flame. This increase of temperature of the surface (to about a red heat when water is being boiled) can be brought about by inserting a properly pro-

portioned thermal resistance between the heat-receiving surface and the boiling liquid. This was shown by the writer in an article on "A New Principle in the Flow of Heat" in the *Journal of the Franklin Institute,* January, 1918, page 75, and another in *Power* for January 1, 1918. In this way the writer has transmitted heat from a flame to water from 25 to 30 times as fast through the same area of surface.

It seems likely that this supposed high-resistance film is not a true thermal resistance, its estimated resistivity being many times that of good insulators like felt, but that the true explanation is that when hot gases impinge on a relatively very cold surface much of the heat is reflected and but little is transmitted. Perhaps the transference of the momenta of the moving molecules constituting heat is the explanation, in which case that part of the energy which is not transmitted is reflected.

Carl Hering

Philadelphia, Pa.,
February 15, 1918

THE AURORA OF MARCH 7, 1918

Those who saw the aurora of August 26, 1916, did not expect to see such a display repeated within a life time, but on March 7, 1918, there was a similar spectacle which from reports must have been visible over practically all of the northern hemisphere of the earth. I first noticed the aurora low down in the north, about 7 P.M., but in half an hour clouds had come, which continued for an hour or so. At 9.30 I happened to be out of doors and saw that something startling was in prospect, as the sky was clear and the aurora was growing rapidly. The general effect and appearance of the display was accurately described by Dr. Tomlinson of our geology department,[1] and I shall limit my account to the determination of the position of the radiant or apparent focus of the auroral streamers. It was very striking that just when the display was at its maximum the streamers seemed to come from Saturn.

In the following notes Central Standard Time, 6 hours slow of Greenwich is used, the position being latitude 40° 6′ north, longitude 88° 13′ west.

9h. 31m. Streamers rising. Cloud-like form in southeast.

9h. 36m. Radiant exactly at Saturn. Half of sky or more covered. To west and over Jupiter a broad band of red, 10° or 15° wide. This is southern edge of the aurora in that direction.

9h. 41m. Radiant 2° north of Saturn.

9h. 44m. Radiant 2° northeast of Saturn.

9h. 46m. Radiant fainter.

9h. 51m. Radiant has about disappeared.

9h. 51m. All of light is now below Polaris.

10h. 38m. Only faint glow low down.

No further display was noted by our observers at the telescope, who worked until several hours after midnight.

Averaging the three estimates, we have that at 9h. 40.3m. the radiant was 1°.1 north and 0°.5 east of Saturn. The magnetic elements for Urbana are: declination 3° 13′ east, dip 71° 5′, determined by Mr. Merrymon in 1917, and kindly communicated by the superintendent of the U. S. Coast and Geodetic Survey. From the ephemeris position of Saturn, we readily find then for comparison:

	Declination	Hour Angle
Magnetic zenith	+ 21°.2	+ 1°.1
Radiant	+ 20°.1	+ 0°.2
Difference	1°.1	0°.9

The result shows that within the error of estimate the apparent radiant or focus of the auroral streamers was at the magnetic zenith, which agrees with what was observed in 1916.

Joel Stebbins

University of Illinois Observatory

From reports it is learned that the aurora borealis seen here on the evening of March 7 was observed at the same hours from New York City to Salem, Oregon, also at St. Louis and as far south as Lat. 36° N., in North Carolina. It was probably seen by observers over an area of greater extent in the United States and occurred also in Europe. This synchronous occurrence seems to indicate a widespread uniformity of the atmospheric conditions which produced it and to offer additional evidence

[1] Science, March 22.

against the belief, formerly held by some, that this phenomenon is due solely to local conditions.

Here the broad central arc formed low in the northern sky, its highest point being about 20° above the horizon and identical in direction with our magnetic north. It first appeared about 8 P.M. and was then most pronounced (though without definite form) in the northwest. In color the light consisted chiefly of white, yellowish green, and dull red but was at no time very bright, though its varied radial streamers attracted much attention. Its maximum was reached about 10 P.M., when it advanced beyond the zenith to a point about 60° above the southern horizon and covered the major portion of the sky.

JOHN E. SMITH

IOWA STATE COLLEGE,
AMES, IOWA,
March 18, 1918

ON the evening of March 7 there occurred at La Crosse, Wisconsin, the finest display of northern lights that the writer has ever seen here. There seems to be no record or remembrance of any equal display. The lights were observed more or less from 7:30 till 12 P.M. The best were seen from 9:45 to 10:45 P.M. During this time shafts of light starting from the horizon would shoot to the zenith. These shafts would be in the north, northeast, or northwest. After these started, others would follow them till three fourths of the heavens were covered with these shafts of light, for they extended south of west and south of east. In the parts of the heavens farthest south the shafts of light were broken just below the zenith but in the other parts of the heavens the shafts were continuous from the horizon to the zenith. The shafts did not flicker or flash as observed at other times but they remained stationary for a period and then died out.

The most remarkable thing was the colors exhibited. After the shafts had been established faint tinges of red appeared which became brighter till the heavens from the northwest to the northeast, and for three fourths of the way from the horizon to the zenith were covered with a bright crimson red glow. The scene was magnificent and never to be forgotten.

The above light forms would stay for some minutes and then they would all die away and leave only a greenish hue in the north. In a few minutes more all would be repeated again. This repetition was noted several times in succession till about 10:45 it all faded into the greenish hue which lasted an hour.

Other colors were observed as yellows and purples. These were seen as faint light toward the zenith but the prominent color was the red which with the definitely formed shafts gave a special character to these northern lights which will be easily remembered by the observers.

G. H. BRETNALL

STATE NORMAL SCHOOL,
LA CROSSE, WIS.

AN OLD RECORD OF ALBINO TURKEY BUZZARDS

THE appearance in SCIENCE at intervals during the past two or three years of accounts by various observers of albino birds has interested the present writer very much, and all the more because he has never been so fortunate as to see such a bird. Recently while reading the voyages of Captain William Dampier, his attention was forcibly called to the account by this keen-eyed explorer which is given herewith in the belief that it may prove of interest and value to some of the readers of SCIENCE.

> Carrion crows are blackish Fowles, about the Bigness of Ravens; they have bald Heads and reddish bald necks like Turkeys. . . . These live wholly on Flesh (and are therefore called Carrion Crows). . . . Some of the Carrion Crows are all over white, but their Feathers look as if they were sullied: they have bald Heads and Necks like the rest; they are of the same Bigness and Make; without any Difference but in Color; and we never see above one or two of these white ones at a time; and 'tis seldom also that we see a great Number of the black ones, but that we see one white one amongst them. The Logwood-Cutters [of Campeachy] call the white ones King-Carrion Crows.

This account is found on page 168 of Volume II. of the 1729 edition of Dampier's "Voyages" as edited by John Masefield and

published by Richards in London in 1906. Specifically the account is found in Dampier's narrative of his "First Voyage to the Bay of Campeachy" which is dated 1676. Dampier was, of all the early English ship captains and circumnavigators, by far the keenest observer. His "Voyages" fairly bristle with the most interesting and valuable natural history notes, and it seems not improbable that if they were better known they might constitute his best bid for fame. His text seems to indicate quite clearly that these "Carrion Crows" are our well-known "Turkey Buzzards," and if so it may be that this is the first and possibly the only recorded occurrence of albinos among them.

E. W. Gudger

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AN OPEN LETTER

My dear Professor Jeffrey: In your delightful volume (excuse the word, but it expresses my feeling) on Woody Plants, you make a division of Archigymnospermæ and Metagymnospermæ on the basis of pollen chamber, fertilization by motile sperms, and presence of cryptogamic wood. In other places you show the close relationship of Cordaitales, Ginkgoales and Coniferales. Could you explain briefly, in this journal, for the benefit of many who will undoubtedly be interested, why you split the gymnosperms between the Ginkgoales and Coniferales rather than between the pithy stemmed Cycadean series and the woody Cordaites-Ginkgo-Conifer series?

Sincerely yours,
Henry S. Conard

Dear Professor Conard: Your open letter has been submitted to me by the editor of Science. My motive in dividing the Archigymnospermeæ from the Metagymnospermeæ on the basis of the presence of antherzoid fertilization and cryptogamic centripetal wood is largely one of expediency, since there are fortunately almost no gaps in the series of vascular plants outside the very considerable one which separates the Angiosperms from the Gymnosperms. The cryptogamic wood (or the *bois centripete*) has been an important criterion for the lower gymnosperms since the days of Renault and Brongniart. Zoidogamy, predicted by Hofmeister for the lower gymnosperms and discovered in the Cycads and Ginkgo by Hiarase Ikeno and Webber is a prominent character on the gametophytic side. The combination of these two accepted criteria makes the line of separation come above the Ginkgoales. The large pith and large leaves which you emphasize were also possessed by many Cordaitean forms. Some of these had leaf bases very fern-like in organization, as described by Dr. D. H. Scott and myself in remains from the lower Waverly of Kentucky. I would repeat that the term Archigymnospermeæ is one of convenience, and like most scientific terms falls short of covering the situation. I would quite agree with you that the Ginkgoales are fully as closely allied to the Coniferales as to the Cordaitales, yet convenience and the present state of our knowledge includes them with the ancient gymnosperms. I may add that your "Cycadean Series" appears to me to be a very natural one, and in fact is generally admitted. Hoping I have made my position clear I remain, Yours sincerely,

E. C. Jeffrey

SCIENTIFIC BOOKS

Studies in the History and Method of Science. Edited by Charles Singer. Oxford, Clarendon Press, 1917. xiv + 304 p. 4°, XLI plates (many colored), 33 illustr. in text.

As Sir William Osler tells us in the introduction to these essays, they are the outcome of a quiet movement on the part of a few Oxford students to stimulate a study of the history of science. Upon the generous initiative of Dr. and Mrs. Charles Singer, a bay has been set apart in the Radcliffe Camera of the Bodleian for research work in this field. The objects pursued are: first, to place at the disposal of the general student a collection that will enable him to acquire a knowledge of the development of science; secondly, to assist the special student in research: (*a*) by placing him in relationship with investigations already undertaken, (*b*)

by collecting information on the sources and accessibility of his material, (*c*) by providing him with facilities to work up his material.

In spite of Dr. Singer's absence on military duty, the work has been carried on with conspicuous success. Ten special students have already used the room: Ramsay Wright has made a study of a Persian medical MS.; Walter Libby of Pittsburgh, during the session of 1915–16, collected material for his well-known book on the history of science; E. T. Withington is investigating the Greek medical texts for a new edition of Liddell and Scott's dictionary. Miss Mildred Westland has helped Singer with the Italian medical MSS. Reuben Levy has worked at the Arabic medical MSS. of Maimonides. Mrs. Jenkinson is engaged on a study of early medicine and magic. J. L. E. Dreyer has used the room in connection with the preparation of the "Opera Omnia" of Tycho Brahe. Miss Joan Evans is engaged upon a research on medieval lapidaries. Mrs. Singer has begun a study of the English medical MSS., with a view to a complete catalogue.

This is a splendid beginning—the right beginning—and one can not bestow too much praise on Dr. and Mrs. Singer for their enthusiastic and contagious activity.

This volume of essays is issued as a *ballon d'essai,* but we earnestly hope that a well-deserved successs will encourage its editor to publish periodically a similar one. That this first one has been issued at all, and "got up" with such admirable scholarship and taste in the fourth year of the war, is a credit to Oxford.

One half of the book is Singer's own work. He has contributed two very important studies. One on the scientific views of Saint Hildegard. The author traces the sources of her knowledge, by no means an easy task, one whose accomplishment implies a great familiarity with medieval science. It is interesting to compare this study with earlier ones devoted to the same subject: it illustrates the whole difference between historical studies of the old literary type and those permeated with the scientific spirit—a genuine understanding of scientific problems and values. The other is a study in early Renaissance anatomy with a new text: the Anothomia of Hieronymo Manfredi (1490). Both studies are very accurate, clear and complete; they are magnificently illustrated.

There is a suggestive paper on vitalism[1]—in fact the most comprehensive short statement of this question which I have read—by the much lamented John Wilfred Jenkinson, (with a portrait). Dr. Jenkinson was killed in action in 1915, at the Gallipoli peninsula. He had already produced excellent embryological work, but was still very young and his friends had placed considerable faith in him.

I must quote more briefly the other papers: Raymond Crawfurd deals with "The blessing of cramp-rings. A chapter in the history of epilepsy." E. T. Withington's essay is devoted to one of the most clear-minded men of the sixteenth century: Dr. John Weyer, the first serious opponent of the witch-mania. Any one acquainted with the history of witch-craft will at once appreciate his greatness. Most historians probably know very little about him, and yet this man was far greater than the contemporary kings and princes about whom they know and tell us so much. And do you think it was by mere coincidence that the first opponents of the witch-mania were scientists? Reuben Levy shows that the "tractatus de causis et indicis morborum" attributed to Maimonides, is most likely not his own work. Lastly there is a long essay by F. C. S. Schiller: "Scientific Discovery and Logical Proof." Interesting as it is, I think that this paper is here somewhat out of place. A book chiefly devoted to the history of science, should only harbor such philosophical and methodological studies as are based on historical information.

The publication of this book is highly gratifying. It proves that in England, as well as in Italy, France and Germany, the idea of the history of science is crystallizing and growing fast.

In America also, the year 1917 has brought to light some important contributions to this movement. I refer to the publication of two

[1] It is a revised edition of a paper published in the *Hibbert Journal,* IX., 545–559, 1911.

remarkable text-books. The first in point of date is Walter Libby's "Introduction to the History of Science" (Boston, Houghton Mifflin), a very well written and interesting account of some of the most typical conquests of science throughout the ages. The second is Sedgwick and Tyler's "Short History of Science" (New York, Macmillan), whose more ambitious purpose is to outline the whole of its development. The authors have been teaching the history of science for a great many years at the Massachusetts Institute of Technology. They have taken great pains to make their book as serviceable as possible to the student. It contains a large number of relevant quotations, and some longer extracts from Hippocrates, Roger Bacon, Copernicus, Harvey, Galileo, Newton, Jenner, Lyell; also notes on the main inventions of the last two centuries, and chronological and bibliographical summaries.

With books like these, the American teachers will have no excuse for not knowing at least something on the subject. It is not unlikely that now that such convenient text-books are provided, courses based upon them will grow like mushrooms; those giving them will gradually become acquainted with the history of science, and some of them will wish to know more of it. That is also a beginning.

The reader should not be left under the impression that no real research work has been undertaken in this country. It will be enough to remind him of the two beautiful studies contained in the eleventh volume of the Humanistic Series of the University of Michigan. The first is an admirable edition of Robert of Chester's Latin translation of the "Algebra" of Al-Khowarizmi, with an introduction, translation, and notes by Ch. L. Karpinski (1915). The second, an English version of Nicolaus Steno's "Prodromus," with notes by J. G. Winter (1916). This volume is to be completed by an essay on the "Vesuvius in Antiquity," contributed by the editor of the series, Francis W. Kelsey.

I believe that no greater service can be rendered to the history of science, at this juncture, than by relentlessly insisting upon the necessity of raising the standard of scholarship as high as possible. It will gradually dawn upon the people that inaccurate historical facts are as worthless as inaccurate scientific facts. It is true, historical errors are less obvious. At least they are not automatically detected, as is the case in the positive sciences, where most errors lead sooner or later to inconsistencies. But does the fact that there is no material check of our accuracy in historical matters not increase—rather than decrease—our duty to be accurate?

The Past can not rise up and arrest the historian, crying out: "You inconsiderate tattler, you liar, how dare you. . . . ?" The historian is a judge. Upon his shoulders rest the immense responsibilities of a judge. And those upon whom he sits in judgment have been silenced forever.

It is not always easy to appreciate the merit of a contemporary, because we can not justly estimate the value of a discovery until we have got far ahead of it, and tasted a great many of its fruits. And so it happens all the time that some people enjoy a very high reputation, which they do not deserve, while others traverse life unrewarded and unnoticed.

From the idealistic point of view, this does not matter much, because, sooner or later, justice is done. That is the historian's trust. If he be unfaithful to it—if he says for instance that the air pump was discovered by Robert Boyle instead of by Otto von Guericke—this is not a mere trifle. It is a protracted injustice; the supreme crime.

But the ultimate purpose of those who are now fighting under the banner of the history of science—only a few to-day but legion to-morrow—is even higher. To put it briefly, their purpose is to reconcile knowledge and idealism. We need both equally. I do not know which is worst, knowledge without idealism or idealism without knowledge, and yet our whole system of education is leading to their growing estrangement.

It would be foolish to imagine that scientists and literary people, the so-called humanists, will come together spontaneously. Such a miracle will not come to pass, unless we

prepare for it. Scientists are too busy to undertake historical studies, and literary people can hardly be expected to reeducate themselves along scientific lines. A bridge must be built. The task of unification must be intrusted to specialists, equally well trained as scientists and historians. There is no other way out. Complete courses on the history of science must be organized, and at least a few men must be given the material possibility of devoting themselves entirely to this work of synthesis.

The books of which I have spoken are new tokens, among many, of the irresistible movement which is leading to the organization of these studies and we must be grateful to all those who are helping—either as scholars or as vulgarizers. It remains to be seen which university will take the lead; the others will follow.

GEORGE SARTON

INVERTEBRATE PALEONTOLOGY

IN 1879 Hall described in a Report of the New York State Museum a curious fossil from the Silurian, giving it the name *Camarocrinus stellatus*. It was a large globose structure, internally divided into several chambers, attached at one end to a stem agreeing with that of the crinoids. Hall surmised that it represented the base of some crinoid species, the other end of the stem, with its calyx, being still unknown. This explanation was generally accepted, but the peculiar chambered bulbous form was supposed to represent a special adaptation for a floating mode of life, the bulb being in fact a float, from which the crinoid hung suspended in the water. In 1904 Dr. R. S. Bassler, of the U. S. National Museum, found an apparently promising locality for *Camarocrinus* a few miles north of Cape Girardeau, Mo., along the bluffs of the Mississippi River. This led Mr. Frank Springer in 1912 to send Mr. F. Braun to the locality, with the result of uncovering the most marvelous specimens of crinoids, completely solving the mystery of *Camarocrinus*, and furnishing the National Museum with a slab about 4 by 5½ feet, covered with remains of the animals. This slab, now mounted in the hall of invertebrate paleontology, will always remain one of the most striking paleontological specimens in existence, for it shows the animals as they died, probably smothered by a deposit of mud brought by a swift current from some higher level. Mr. Springer has prepared a detailed and beautifully illustrated account of the new materials, and has taken occasion to review all the congeneric species found in America.[1]

It turns out that *Camarocrinus* is the basal end of a well-known crinoid, described first from Bohemia by Zenker in 1833. Zenker called it *Scyphocrinites*, and according to the rules of nomenclature this is the proper name, but students of crinoids have chosen to shorten such names, in the present case writing *Scyphocrinus*. The Missouri slab proves to belong to Zenker's original species, *S. elegans*, the American specimens agreeing in all respects with those from Bohemia. The basal bulb was not afloat at all, but was embedded in the mud, acting as a root. In its resemblances to and differences from the calyx end it suggests some interesting reflections in connection with the researches of Professor C. M. Child, of Chicago. To what extent is it purely adaptive, and how much of its form and structure depends upon a principle of partial repetition of the fundamental structure of the animal?

Although the Missouri slab represents the Bohemian species, Mr. Springer finds that there are no less than seven other forms of *Scyphocrinites* (or, as he writes it, *Scyphocrinus*) in American rocks. *S. stellatus* is Hall's original *Camarocrinus; S. pratteni* is a very fine species from Tennessee, described under another generic name by McChesney as early as 1860; *S. ulrichi* was described by Schuchert as a *Camarocrinus;* and finally *S. spinifer, S. mutabilis, S. pyburnensis* and *S. gibbosus* are published as new by Mr. Springer.

The Carboniferous (Pennsylvanian) rocks of Maryland, Pennsylvania, West Virginia and Illinois have in recent years yielded a most interesting assemblage of fossil insects, which

[1] Frank Springer, "On the Crinoid genus *Scyphocrinus* and its Bulbous Root *Camarocrinus*, Washington, Smithsonian Institution, 1917, 74 pp. and nine plates.

seem likely to afford important aid in working out the stratigraphy, and also to throw new light on the relationships between the faunæ of the old and new worlds in Carboniferous time. Many of the American species, though described and figured, still await publication. In the meanwhile, it is satisfactory to find that, in spite of the war, the insects of the European coal-measures are being fully described and well figured by Mr. Herbert Bolton, of the Bristol Museum. The apparent poverty of England in fossil insects of Paleozoic age may prove illusory, due merely to lack of interest in their discovery and identification. At all events, Mr. Bolton is making known a number of new types, his latest paper on this subject[2] containing accounts of new species, and a very fine new genus, for which he unfortunately uses the preoccupied name *Palaeomantis*. In another paper[3] Mr. Bolton gives an account of some insects from the coal measures of France, describing among other things the singular new genus *Megagnatha*, which appears to have long slender jaws, recalling those of the modern *Corydalis* male. The name *Megagnathus* has been applied to a genus of beetles, but it is to be hoped that no one will think it necessary to alter the name of Mr. Bolton's genus. Those who propose to change names in zoology, merely because they are thus similar, can hardly understand what confusion would result from the universal application of such a method. We all agree that absolute homonyms can not be permitted to stand, but a difference in the ending of the word is abundantly sufficient to prevent confusion. In connection with the evolution of cockroach types in America and Europe, particular attention must be called to Mr. Bolton's *Neomylacris lerichei*, which certainly has a strongly American facies. I should call it *Promylacris lerichei*, referring it to a genus described from Mazon Creek, Illinois.

[2] "On some Insects from the British Coal Measures," *Quart. Jour. Geol. Society*, Vol. 72 (1916), pp. 43–62.

[3] "The Mark Stirrup Collection of Fossil Insects from the Coal Measures of Commentary (Allier), Central France," *Mem. Manchester Lit. and Phil. Society*, May 11, 1917.

While the European Paleozoic insects are thus being elucidated, very welcome information reaches us from Australia, of the discovery of a rich fauna of Mesozoic insects.[4] It appears that the specimens represent a Triassic fauna, and consequently tend to complete our ideas of insect phylogeny, filling in a gap which has hitherto existed in the record. One of the Neuropteroid faunas is referred by Mr. Tillyard to a new order, Protomecoptera, having the surface of the wings finely reticulated. In modern *Panorpa* traces of the primitive reticulation can be seen in a good oblique light, and it really seems unnecessary to recognize more than a superfamily (Archipanorpoidea).

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SPECIAL ARTICLES

THE EVOLUTION OF BACTERIA

Dr. I. J. Kligler has recently contributed a paper on "The evolution and relationship of the great groups of bacteria."[1] The conclusions reached are so surprising and so in conflict with commonly held opinions that the contribution is well worthy of careful scrutiny to determine whether or not the premises are reasonable, and the deductions logical. It appears to the writer that in several instances Dr. Kligler has failed to prove his points and that his conclusions, at least when based upon the premises and the reasoning used, are open to serious question.

Probably no fault can be found in the introductory statement that the bacteria are "among the most primitive of living forms." It should be remarked, however, that this does not prove that the present living bacteria are any of them identical with, or even closely related to, the original types of bacteria which appeared upon earth. The next statement can not be accepted quite so readily. The author says:

[4] R. J. Tillyard, "Mesozoic Insects of Queensland," *Proc. Linn. Soc. New South Wales*, July 11, 1917. Also publication 253, Queensland Geological Survey (1916).

[1] *Journal of Bacteriology*, March, 1917.

The ability of some types to subsist on simple inorganic substances (CH_4, NH_3 and CO_2) without the aid of sunshine, and the sensitiveness of all bacteria to the action of sunlight suggest their existence on this planet prior to the appearance of plant life or the penetration of the rays of the sun through the volcanic vapors.

We may perhaps agree that an organism which can develop without organic compounds of any kind, utilizing inorganic compounds exclusively, would probably be primitive. There is no proof, however, that the original bacteria were any more sensitive to the sun's rays than are plant cells in general at the present day. It is not improbable that the purple sulphur forms are among the most primitive of our modern bacteria, and these, as is well demonstrated by the work of Molisch and others, grow only in the presence of light, and the motile forms show a very marked positive phototaxis. Most animal cells are equally sensitive to sunlight with most bacteria, but this does not argue that these animal cells are primitive. Finally, the assumption that life must have originated on earth while the earth was still bathed in volcanic vapors, and the earth's surface was dark, has little to substantiate it. It is quite as probable that we must account for the development of life upon earth on the basis of Chamberlain's accretion theory as on the theory of a once molten globe. It is not argued that either hypothesis of earth origin must be accepted, but Dr. Kligler's assumption is rather an insecure basis for the erection of his complex superstructure.

It is rather difficult to follow Dr. Kligler's reasoning through the succeeding paragraph. The thesis to be proved apparently is: "The intimate dependence of both plants and animals on bacteria and their activities tends to strengthen the conviction that these microorganisms must have preceded the others." The statement may well be accurate, but the examples adduced are not in all cases fortunate. For example: "Plants can not subsist without nitrates" is misleading, for many of the higher plants, fungi, yeasts, algæ, etc., utilize ammonia quite as well as nitrates, in some cases utilizing ammonia in preference. To state that in arid regions where plant life is absent bacteria are also absent is simply to affirm that both bacteria and higher plants require water for their growth, or are killed by an excess of salts in the soil rather than that the higher plants are dependent upon the bacteria. The author's examples of the necessity of bacteria for the growth of animals are likewise not convincing as proof.

Attention is next called to the fact that with the bacteria evolutionary changes may be physiological as well as morphological, and the point is made that these "adaptive modifications" should be traced.

The statement that "Bacteria need only minute amounts of nitrogenous food but require a relatively enormous quantity of energy-yielding (carbon) compounds" is doubtless true for many of the fermentative types, but there seems to be no more reason for this assertion for primitive bacteria than for higher plants, or the fungi. Then it is argued since most bacteria, at least the saprophytes, can utilize nitrogen either as NH_3, or NO_3 and show the most diverse ability to use various carbohydrates, then the first path of early evolution must have been increasing ability to use a variety of carbohydrates, and only later, when a higher scale of development is reached do we find increasing power to utilize complex nitrogenous compounds. No evidence is adduced that the primitive organisms were furnished with abundance of carbohydrates before they had opportunity to attack considerable quantities of protein or other complex nitrogenous substances. It should be emphasized that carbohydrates in the sense used and in quantity needed would probably be produced only by higher plants. But all plants contain protoplasm, and in their decay organisms have access to proteins as well as to carbohydrates. In spite of the array of supporting evidence, it seems that there is no adequate proof that utilization of carbohydrates is any more primitive a function than is proteolysis.

The point is made that there are four principal types of primitive oxidizing bacteria,

those which oxidize C, N, S and Fe, then follows the statement:

> It seems fairly certain from the important part played by carbon compounds in the vital activities of our common bacteria, especially as a source of energy, that the carbon oxidizers are the forerunners of the bacteria of to-day.

The author then concludes that the most primitive types must be those capable of oxidizing methane, followed by those which oxidize carbon monoxid, and, most astounding, those capable "of utilizing CO_2." A careful scrutiny of the text will lead to no conclusion but that "utilizing" is here used to indicate "oxidizing." This inference is strengthened by a second statement even more surprising.

> Since the ammonia and nitrate oxidizers (or nitrifiers) also assimilate large amounts of carbon-dioxide (Jensen) they would seem to fall in line along with the organisms capable of *obtaining their energy from carbon dioxide.*[2]

By what mystic process an organism may secure energy from CO_2 is not explained. The assumption that carbon-oxidizing forms are most primitive is scarcely proved. In fact, study of the oligocarbophilous organisms, that is, forms which can utilize CO_2 in the building up of food or protoplasm indicates that the more primitive of the modern types may be among the nitrifying and the sulphur bacteria. An analysis of the conditions upon the earth in early times as postulated by the author and elaborated by Osborn would seem to indicate that they would be even more favorable to the development of ammonia or sulphur oxidizers than for methane oxidizers. It should be emphasized that all organisms of types not using organic carbon must have some source of energy which will enable the protoplasm to take up CO_2, replace the oxygen in part with hydrogen and build up complex organic compounds. The energy for this transformation might have come from the oxidation of ammonia or sulphur. Certainly ground waters laden with hydrogen sulphide must have reached the surface of the primitive earth much as they do to-day. Furthermore, the consistent development of the pigment bacteriopurpurin by most members of the modern large group of sulphur bacteria, together with the marked phototaxis of this group, might be interpreted as evidence that the sun's rays had some influence almost from the beginning in the explanation of energy source in CO_2 assimilation. In other words, early coordination of photosynthesis with chemosynthesis can not be ignored as a possibility.

It may be noted further that to discuss primitive bacteria as capable of utilizing formic acid, acetic acid and alcohol is somewhat anachronistic. These substances are the results of the growth or fermentative power of organisms which stand higher in the scale of evolution. Where could the primitive organisms find these complex compounds to work on?

The author next calls attention to the fact that certain organisms changed their habit of life from that of obligate aerobes, using atmospheric oxygen, to that of facultative anaerobes, "utilizing combined oxygen for intracellular combustion." The statement is made that the "prototrophic denitrifying bacteria are most probably the progenitors of this group." The statement requires some analysis. One would infer that prototrophic denitrifiers are common and well known. As a matter of fact, the denitrifying organisms are almost without exception anything but prototrophic. When one wishes to demonstrate denitrification in the laboratory it is customary to add a suitable organism to an aqueous solution of nitrate and organic carbon compounds. Under anaerobic conditions the oxygen is removed in whole or in part from the nitrogen, and used for oxidizing the carbon compounds present. The only exception to this rule known to the writer is the peculiar organism described by Beijerinck which will grow in the presence of free sulphur, nitrate and carbon dioxide in the absence of atmospheric oxygen, oxidizing the sulphur and using the energy thus gained apparently in the assimilation of CO_2. It would seem that such a form would be much more closely related to the *Thiobacteria* than to the other denitrifiers. To make his point, Dr. Kligler should cite some examples of "prototrophic denitrifiers."

[2] *Italics not in original.*

If descriptions of such organisms are nonexistent in the literature is it necessary to assume that they have not been discovered or that they once existed but have disappeared? Is it so evident that

> The line of descent from the prototrophic denitrifiers is entirely clear.

The statement is made that next in series following the prototrophic denitrifiers probably came the *aerogenes* type of organism, one indication of relationship being that it can live "in simple inorganic media and under certain conditions, even to fix atmospheric nitrogen." The present writer has been unable to find any evidence that the *aerogenes* group can ever grow in a medium devoid of organic matter. It is true that inorganic nitrogen compounds may be used, but this, as was above indicated, has no particular significance, as many bacteria, yeasts, molds, algæ and even flowering plants have the same power. Any close relationship of *aerogenes* to a prototrophic form certainly has not been proved. However, the author's claim for close relationship between *aerogenes* and *coli* and through the series to the typhoid, dysentery and possibly even to the hemorrhagic septicemia groups has much to commend it, and is a generally accepted hypothesis at present among bacteriologists.

It is possible that the relationship assumed to exist between *B. proteus* and the spore-bearing rods is a real one, but the structural modification incident to the development of the power of endospore production is so great that mere association of organisms in putrefactive processes and common property of producing proteolytic enzymes does not prove close affinity. It should be borne constantly in mind that proteolytic enzymes are commonly produced by cells. Every cell has protoplasm consisting largely of protein. Cells in general after death undergo autolysis. Every cell then contains an autolytic proteolytic enzyme. The step to the production of an extracellular proteolytic enzyme would therefore seem not to be a difficult one to make, and one which may have been made independently by different groups. It might be mentioned, however, that Kligler has ignored one point of morphology which would tend to show relationship better than proteolysis, this being the diffuse arrangement of the flagella in both groups. Apparently organisms showing peritrichous flagella constitute a group rather distinct from the forms with polar flagella.

The type of pigment produced, and the general cultural and physiological characters would seem to argue quite as close a relationship between the *Rhodococcus* and *Micrococcus* and the rods producing red or yellow pigment, as between the latter and the spore-bearers or *B. proteus*. Many more resemblances than differences will be found, for example, between *Rhodococcus roseus* and the organism usually known as *Bacillus prodigiosus*.

The effort to derive the gram positive streptococci from the aerogenes types seems rather strained. Differences are more marked than resemblances. The products of fermentation are very distinct in the two groups. This fact together with decided differences in morphology, relationship to oxygen and cultural characters, counterbalance the presence of capsules of similar composition, ability to ferment inulin (in fact a variable character in *aerogenes*) and localization in the same organs of the animal body. However, it should be granted that the author's emphasis on relationship between the streptococci and the aciduric bacilli is probably well placed.

Kligler's characterization of *Azotobacter* as a form not only fixing atmospheric nitrogen but oxidizing nitrogen to nitrates is apparently not well founded. It may be noted that there is an apparent tendency to consider *Azotobacter* prototrophic, an assumption which is quite unwarranted. The statement "the *Azotobacter* can assimilate free nitrogen more readily if glucose and a small amount of ammonia are supplied" is misleading. Apparently *Azotobacter* is wholly incapable of growing, certainly incapable of fixing atmospheric nitrogen, without an abundance of soluble carbohydrate food. By oxidation of this carbohydrate, energy is secured for the fixation of sufficient atmospheric nitrogen for the needs of the cell, but there is apparently no nitrogen changed into the form of nitrates.

Furthermore it should be emphasized that this organism has very high fermentative powers, producing large amounts of CO_2 and H_2O.

That the organisms of the nodules of the legumes are closely related to *Azotobacter* is not improbable, but that there is any close relationship between *Rhizobium* and the acid-fast bacteria and the *Actinomycetes* is not so clear. It is true that the latter contention has been supported by several writers, but the fact that *Rhizobium* produces nodules on the plant roots and the tubercle bacillus causes tubercles to develop in animal tissues is no more of an argument for their inter-relationship than to claim that the nematodes producing galls on plant roots are related to the tubercle bacillus or to the *Rhizobium* for the same reason. The differences between the motile (polar flagellate) gram negative, *Rhizobium,* fixing atmospheric nitrogen, and the acid-fast gram positive, non-motile tubercle bacillus incapable of fixing nitrogen are very marked and tend to outweigh the remote resemblance of the branched bacteroids to branched tubercle bacilli.

The relationship indicated between the tubercle bacillus and the *Actinomycetes* is not at all improbable, in fact, intermediate forms have been described.

The use by the author of the name *Sporothrix* for a group of bacteria is unfortunate, and will tend to confusion.

It is surprising to find the *Micrococci* and *Staphylococci* at one extreme and *Streptococci* at the other of the classificatory scheme that is worked out. The concept is unusual, to say the least, and is scarcely supported by adequate proof to be convincing.

It would seem that the family tree of the bacteria suggested by Dr. Kligler is based upon many misconceptions and misinterpretations and can scarcely be accepted without much more adequate proof. However, there is much in the article to provoke thought and discussion, and if this is accomplished and some conclusions eventually reached, the effort put forth can scarcely be said to have been in vain.

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Iowa State College

THE AMERICAN MATHEMATICAL SOCIETY

The one hundred and ninety-sixth regular meeting of the society was held at Columbia University on Saturday, February 23, extending through the usual morning and afternoon sessions. Seventeen members were in attendance. Professor H. S. White occupied the chair. The following were elected to membership: Miss M. F. Chadburne, Smith College; Mr. Mervyn Davis, Equitable Life Insurance Company of Iowa; Mr. T. C. Fry, Western Electric Company; Dr. J. E. McAtee, University of Illinois; Dr. Norbert Wiener, Albany, N. Y. Four applications for membership were received.

The following papers were read at this meeting:

J. F. Ritt: "Proof of the multiplication formula for determinants by means of linear differential equations."

Olive C. Hazlett: "On vector covariants."

P. R. Rider: "On the problem of the calculus of variations in n dimensions."

A. R. Schweitzer: "On the iterative properties of an abstract group."

A. R. Schweitzer: "On certain articles on functional equations."

A. R. Schweitzer: "On iterative function equations."

J. R. Kline: "A new proof of a theorem due to Schoenflies."

R. L. Moore: "A sufficient condition that a system of arcs should constitute a surface."

J. L. Walsh: "On the location of the roots of the Jacobian of two binary forms, and of the derivative of a rational function."

O. E. Glenn: "Covariant expansion of a modular form."

J. F. Ritt: "Polynomials with a common iterate."

L. P. Eisenhart: "Transformations of applicable conjugate nets of curves or surfaces."

S. E. Slocum: "The romantic aspect of numbers."

The San Francisco Section of the society will hold its semi-annual meeting at Stanford University on April 6. The Chicago Section will meet at the University of Chicago on April 12–13; this meeting will include a symposium on divergent series and modern theories of summability. The regular New York meeting will be held at Columbia University on April 27.

F. N. Cole,
Secretary